Introduction

This is an unusual book in that it is not simply a revised or updated edition of a work that in certain quarters has become well known; it is really two books in one. The first part contains the original text of *The Power of Power Politics*: *A Critique*. This provides a theoretical intellectual history of international relations inquiry, applying and testing several propositions about scientific disciplines initially presented by Thomas Kuhn (1962). Its argument is that realism, specifically the work of Hans J. Morgenthau, has provided a paradigm for the field that guides theory and research. It then goes on to review systematically the statistical findings in the field to show that the paradigm has not been very successful in passing such tests and concludes that this evidence along with well-known conceptual flaws indicates that the realist paradigm is a fundamentally flawed and empirically inaccurate view of the world.

Since the original text has acquired a life of its own, I have not sought to revise it so as to make the views of someone who was starting out in the profession accord with someone who is now in his mid-career. It is published as it was in its first printing except for the deletion of a few minor citations and about eighty pages from chapter 4 – pages which provided a detailed review of international relations theory in the 1950s and 1960s but which is less relevant now. This slight abridgement actually makes the text closer to the dissertation that gave rise to it in that the main revisions were in chapter 4 and the addition of chapter 8, which provided a new conclusion.

Nor was it ever my intention to truly update the text. Done properly that would involve new data analyses that would essentially replicate chapters 4–7. That would require an immense effort and is certainly worth doing, but it is not clear that this sort of additional evidence would change anyone's mind about the argument, even though a key part of the argument is empirical.

The reason for this is that an increasing portion of the field, even within North America, has, until quite recently, moved further and further away from quantitative analysis. The crest in this anti-quantitative sentiment was perhaps reached with the publication of Puchala's (1991) ''Woe to the Orphans of the Scientific Revolution.'' Since then the tide has turned the other way with the findings on the democratic peace increasing the interest in scientific research even among senior scholars who had long been hostile to such modes of analysis. It was these non-quantitative scholars whom I wanted to reach, and I knew that another data-based analysis would not do it. Many of these scholars had already reacted to the quantitative evidence presented in the original text by saying that all that this indicated was that quantitative analysis is a flawed method that cannot produce knowledge; not that the realist paradigm is inaccurate. I therefore decided to employ a mode of analysis more amenable to them and to focus on current non-quantitative theory and research. This research, which is often conducted by realists, but not confined to them, is best known for its use of comparative case studies, historical analysis, and theoretical argumentation, while at the same time eschewing quantitative analysis. Because the roots of its work can be traced back to Hedley Bull's (1966) defense of traditionalism, I have labeled this approach *neotraditionalism*. Among the major journals neotraditionalists dominate are *International Security*, *International Organization*, *Political Psychology*, and *Security Studies*.

Among realists, this approach reflects a third generation of contemporary scholars working within the central core of the realist paradigm, with Morgenthau and the early realists (like E. H. Carr, Reinhold Niebuhr, and George Kennan) being the first generation and the neorealists Kenneth Waltz and Robert Gilpin being the second. Within North America the third generation of realists include John Mearsheimer, Stephen Walt, Joseph Grieco, Randall Schweller, Michael Mastanduno, and Barry Posen, as well as those, who, while critical of certain aspects of realism, remain within that larger paradigm. Most prominent among these are Jack Snyder, Kenneth Oye, and Stephen Van Evera. There are also a number of nonrealists who reflect a neotraditional orientation in their research and mode of discourse, i.e. an emphasis on history, case studies, and a de-emphasis on quantitative findings. Among those who have pioneered the case method are the more senior Alexander George, as well as third-generation scholars Richard Ned Lebow and Janice Stein.

The best way to address the objection that the conclusion of the original text could not be accepted (because it focused on quantitative findings and quantitative scholars) was to look at non-quantitative research. Examining this research would be a logically compelling way of demonstrating that the anomalies the realist paradigm needs to explain away are not exclusively associated with the use of a particular method. Empirical research that is done well should not produce different results depending on the research techniques employed; statistical, historical, and comparative case studies should produce convergent findings.

In addition to examining non-quantitative research, I wanted to provide at least an overview of international relations theorizing in light of the changing intellectual and historical context of the last two decades of the twentieth century. The original text had been written before the rise of neorealism; before post-positivism, post-modernism, and feminist discourse; before the end of the Cold War; and before the widespread attention devoted to findings on the democratic peace and the concomitant rise of the liberal Kantian paradigm. How did these movements and events affect the claims made for and against the realist paradigm in the original text?

At the same time, I felt the need to appraise the quality of realist theorizing, especially since one of the claims in favor of the realist paradigm was that it was, by far, more theoretically robust and fruitful than possible alternatives. I also wanted to examine the connection between realist theory and realist practice. If it were true that the realist paradigm was both as dominant and as fundamentally flawed as argued in the original text, then this should have some impact on realist ability to provide an understanding of contemporary events and guide practice. It was my suspicion that neotraditionalists make their greatest errors when they ignore all research and seek to deduce knowledge on the basis of realist understandings and then use this "knowledge" to derive policy prescriptions.

Obviously, such an agenda was much too ambitious, and I settled instead on doing some carefully selected case studies on the most important questions. The end result is a sequel to the original text that constitutes Part II of this volume. This "new" text complements the original both historically and logically. Historically, it traces and appraises the major trends in realist work from Waltz (1979) through neotraditionalism; it examines the rise of post-positivism and post-modernism in terms of its implications for paradigm evaluation, and it looks at the impact of the major historical event of the current era – the end of the Cold War – on realist inquiry.

Logically, this second part is meant to complement the first by employing a different and new body of theory and research and by applying a broader set of criteria to evaluate the paradigm. If this effort is to be successful, the logic of this research design and how it complements that of the original text must be made explicit. In terms of comprehension of coverage, the original text examined classical realism and quantitative international politics, and the sequel examines neorealism and neotraditional research. In this manner, all relevant realist variants are covered and both quantitative and non-quantitative evidence is included.

Unlike the original text, where all statistical findings in a given period were analyzed, not all neotraditional research will be examined. Instead, case studies of different areas of inquiry will be selected, and then the most appropriate research will be brought to bear to deal with the criteria being applied. In order not to bias the results, it is important that the topics of inquiry that are selected be central to the realist paradigm and indicative of some of the best work done on realism. An evaluation of peripheral areas or of straw men will not do much to reaffirm the original thesis on the inadequacy of the realist paradigm.

Identification of the most important and best realist work in the last fifteen to twenty years is not difficult and is not very controversial. Clearly, the single most important work in terms of its intellectual impact on the field, the attention it has received, the research to which it has given rise, and its use to inform policy analyses has been Waltz's (1979) *Theory of International Politics* (see Buzan et al. 1993: 1). To this, one might want to add Gilpin (1981), the most politically oriented (as opposed to economic-oriented) of his works. Together these are the heart of neorealism and respectively have informed much of realist-guided work in international politics and international political economy. Since the former is of main concern here, only that aspect of Gilpin's work that has had a major impact on questions of war, peace, and political conflict will be included. Because neorealism has been such a major force within the field, it was decided to devote an entire chapter to it to see what this new theoretical version of realism could tell us about the power of power politics thinking to guide inquiry and accurately explain phenomena.

Waltz (1979) focuses on two major subtopics of inquiry: an explanation of what he regards as the major law in international politics – the balancing of power – and an analysis of the comparative stability of bipolarity and multipolarity. Each of these was selected as a focus of separate case studies, once it was determined that a body of relevant research or discourse had been devoted to them by prominent neotraditionalists.

Waltz's ideas about balancing of power have actually spurred a great deal of neotraditional research and theoretical innovation in light of that research. Research by Stephen Walt (1987), Christensen and Snyder (1990), Schweller (1994), Rosecrance and Stein (1993), and the historian Paul Schroeder (1994a, 1994b) has been quite extensive on the questions of balancing, bandwagoning, chain-ganging and buck-passing. In fact, one could argue that this has been one of the most researched areas by neotraditionalists in the last several years. For this reason alone, it is worthy of a case study. In addition, the rise of neorealism and this subsequent theoretical growth have been widely lauded and seen by many as an indicator of the fertility of the realist paradigm and a satisfaction of Lakatos's criterion that research programs should be progressive (see Hollis and Smith 1990: 60).

The work on multipolarity and bipolarity has produced considerably less neotraditional research, but it has been the focus of a major debate about the future of the post-Cold War world. John Mearsheimer's (1990a) article used Waltz's analysis in a theoretically insightful fashion to make predictions and policy prescriptions about the coming multipolar world that attracted wide attention and spurred debate among neotraditionalists. Subsequently, he used realism proper to attack the ''false promise'' of liberal institutionalists' prescriptions of peace. Although many have disagreed with Mearsheimer's (1990a) policy advice, no one has claimed he has misused Waltz or provided an illegitimate version of realism. Given the prominent attention his work has received within the field and its influence outside the field (see Mearsheimer 1990b, 1993), his work was taken as the focus for another case study. This also provided an opportunity to examine how realists use theory to guide practice and to evaluate the empirical soundness of that policy advice.

The case studies on neorealism, balancing, and polarity cover the major intellectual currents within realism and neotraditionalism. There remains, however, one other major intellectual debate relevant to realism and the paradigm debate – the debate spurred by the end of the Cold War. Even though it is not directly related to Waltz's work proper, it does involve that of Gilpin (1981), which is the main realist text used by neotraditionalists to explain the end of the Cold War (see Oye 1995: 58). Beginning with Gaddis' (1992/93) indictment of the entire profession for failing to anticipate the end of the Cold War and the collapse of the Soviet Union, a debate quickly developed over the failure of realism and neorealism to provide an adequate explanation of the Cold War (Lebow and Risse-Kappen 1995). Since realism has placed great emphasis on the ability of international relations theory to comprehend and explain historical events, like World War I and World War II, and rose to ascendency in many ways because of the failure of idealism to prevent the coming of World War II, it was felt that including a case study of the ability of realist and non-realist theories to explain the major historical event of our own time was highly appropriate.

Four intellectual topics, then, will serve as the sample, so to speak, for the case studies – neorealism, neotraditional research on balancing power, Mearsheimer's work, and the debate over the end of the Cold War. This seems to be a representative sample of the most important work in realism since 1979, includes the most prominent thinkers on security questions, and does not leave out any work that would bias the study against the realist paradigm.

Certain areas, of necessity, could not be covered, even where they might be relevant to the major thesis of the book. I have confined the "second part of this volume" to inquiry that has focused on the central questions defined by the realist paradigm – the study of war, peace, conflict, and the foreign policy of "high politics." I have done this because one of the points I want to make is not just that an alternative nonrealist paradigm would look at different questions, but that it would frame realism's central questions in a manner that would provide better and more empirically accurate answers. For this reason, as well as my own expertise, I do not, on the whole, deal with the now rather vast literature on international political economy. This is not too serious an omission because much of the debate over realism in this area of inquiry has been adequately covered in the literature (see, for example, Baldwin 1993).

For reasons of space, I have not been able to go beyond an epistemological discussion of post-modernist approaches in chapter 10. This is regrettable because the theorizing and research of post-structuralists has been one of the more innovative and imaginative areas of inquiry in the last ten years. Similarly, initial criticisms of realism and patriarchy by early feminists, especially those that relied on deconstruction as a technique, have provided some new insights (e.g., Tickner 1992; Cohn 1987; Sylvester 1994), but I have not been able to give this literature the full attention it deserves. At some point, however, feminist discourse in international relations will make an interesting case study of the difficulties of fulfilling critical theory's research agenda in the context of a broader political movement and of balancing concerns about self-interest with the search for truth – ethical and empirical. Nevertheless, omission of research not included in the study – political economy, post-modernist, and feminist research – should not bias the results against the realist paradigm.

The next major question to be decided is what criteria to select to evaluate the realist paradigm. This poses a major epistemological problem because many post-positivists and most post-modernists would object to the kind of scientific (positivist) appraisal conducted in the original text. This necessitates a chapter that comes to grip with the post-modernist and post-positivist critiques. In chapter 10, I discuss the promise of post-modernism and review some of its major insights about theory. I then raise the question of the danger of relativism posed by post-modernism and of the need for theory appraisal. In the chapter, I attempt to reconstruct the foundations of the scientific study of world politics, broadly defined, and to offer a number of criteria for the appraisal of empirical and normative theories. I concede to post-positivists that such criteria cannot be logically justified, but following Lakatos (1970) and Toulmin (1950) I argue that there are ''good [instrumental] reasons'' for *choosing* them, even if scholars are not logically compelled to do so. These criteria then serve as a basis for the paradigm evaluation in the case studies. In order to make the chapters reflect the chronological order of the history of the field, the chapter on post-modernism follows the chapter on neorealism.

The original text employed only one criterion for evaluating paradigms – the ability to pass empirical testing – although it recognized the existence of several. While this criterion must always be at the center of any serious appraisal, I wanted to supplement it with others in the second study. In particular, I wanted to have at least one case study applying the most important of Lakatos' (1970) criteria not applied in the original text – the idea that research programs must be progressive, as opposed to degenerating. Not all bodies of research are amenable to appraisal with this criterion, because in order to apply this criterion, there has to be a considerable body of research available, and it needs to be fairly cumulative. Mearsheimer's work cannot be evaluated along these lines simply because very little neotraditional research has been conducted on multipolarity. Conversely, neotraditional research on balancing of power is an excellent case in which to examine the question of theoretical fertility and progressive/degenerating research programs: first, because the non-quantitative research has been extensive and individual works attempt to build on each other in a cumulative fashion, and second, because this work is often cited as a strength of the paradigm. This criterion will be employed in chapter 11 and provides one of two major studies on whether non-quantitative work will expose realist theories as inaccurate and inconsistent with the evidence.

Mearsheimer's (1990a) work on multipolarity deals with the possibility of peace and the risk of war in the future; it is the focus of chapter 12. Since he uses theory to derive important policy prescriptions, the most appropriate criterion to apply is the criterion of empirical soundness, which maintains that the empirical theory upon which prescriptions are based must be empirically accurate (see ch. 10 in this volume). Unfortunately, there has not been much non-quantitative work on this question or on the question of the effect of norms and institutions on peace. However, there is a considerable amount of quantitative research, and this is consistent with what is known historically about the pertinent periods. Although the use of this evidence makes this case not relevant to the question of whether non-quantitative research will produce the same results as quantitative work, the differences in nonrealist and realist predictions about the immediate future sets up an important "real world" crucial test to resolve this debate. In the meantime, this case exposes the danger of relying too heavily on theoretical deduction and ignoring an entire body of research.

The debate on the end of the Cold War also brings together empirical and policy themes. Here, the most appropriate criteria for theory and paradigm appraisal are explanatory power and relevance. Can the realist paradigm provide a plausible explanation for one of the major historical events of our time and can it provide an intellectual understanding that is *relevant* to the new historical era we seem to be entering? These are the main questions addressed in chapter 13. Non-quantitative and neotraditional research and argumentation are the evidence used to analyze this question, thus providing a second case study to see whether non-quantitative evidence will produce a different conclusion from statistical evidence.

The new text applies the following criteria to appraise the adequacy of recent realist theories, explanations, and prescriptions: empirical accuracy, theoretical fertility (progressive vs. degenerating research programs), empirical soundness, explanatory power, and relevance. In the late 1940s, classical realism claimed to do well on all of these criteria. The original text claimed that quantitative testing raised serious questions about the empirical accuracy of the realist paradigm, as well as pointing out numerous conceptual flaws that weakened its explanatory power. Neorealism and neotraditional realists claim once again to satisfy all of these criteria, and certainly to satisfy them better than any non-realist alternative. The case studies in the new analysis attempt to provide some non-quantitative, but rigorously derived, evidence relevant to each of these criteria. In doing so, it will not provide evidence as systematic as that in the original text, but it will raise a greater variety of questions and potential anomalies than were raised in the original book.

No single case study can ever be definitive; this is a defect of the case study method. Nevertheless, several case studies are more conclusive than one or two. Chapter 14 looks at the collective impact of the case studies conducted in this book for appraising the merits of the realist paradigm and its various branches that have been investigated in the new study. It then reviews the cases and the original text for what they suggest about the promise of a nonrealist paradigm and what problems a nonrealist paradigm would need to resolve in order to produce better and more accurate theories than the realist paradigm has produced. Problems with the major alternative to realism – the Liberal Kantian paradigm – are surveyed. The chapter concludes with a plea for a closer connection between theory construction and research and some ideas to make each more rigorous.